LETTERS TO THE EDITOR.

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Drainage and Malaria.

THE writer of the paragraph upon Drainage and Malaria in the "Notes" of NATURE for September 15, 1910, is evidently unaware that the facts as they relate to Klang and Port Swettenham, the two stations mentioned, lend themselves to a quite different interpretation from that put forward. The idea conveyed by the paragraph referred to is that two intensely malarious places have been freed from malaria by drainage alone, with a saving of more than 400 lives per annum.

A study of the reports that have been published from time to time by Drs. Travers and Watson, the medical officers who claim to be responsible for this remarkable achievement, reveals the following facts.

Klang is the principal town in a district of that name in the Federated Malay States. Port Swettenham is a new port situated five miles from Klang. It was opened in 1901. The total population of Klang district for 1901 was 18,110, of which 3576 belonged to Klang town. In 1903 the population of Klang and Port Swettenham together was estimated at about 4000.

From 1898 to 1904 the total deaths registered in the town and district together were as follows:—

	1898	1899	1900	1901	1902	1903	1904
Total deaths ...	475	558	780	998	547	543	612
Rate per 1000 ...	26·3	31·0	43·0	55·4	30·3	30·1	34·0

It will be seen from this table that the mortality rose very markedly in 1900 and 1901, and fell sharply in 1902.

An analysis of the returns shows this to have been due to an extraordinary temporary increase in the mortality of Klang town during these two years. The following table gives the mortality in the towns from 1900 to 1904 compared with that of the rest of the district:—

		1900	1901	1902	1903	1904
Towns	Total	474	582	144	115	122
	Per 1000 ...	132·5	162·7	36·4	28·7	30·5
District	Total	306	416	402	428	490
	Per 1000 ...	21·8	29·7	28·7	30·5	35·0

A consideration of these figures indicates that some special influence must have been at work in Klang town to cause the appalling mortality of 1900–1, and the history of local events gives the clue to this.

At one time Klang was both port and railway terminus of the district, but in 1897 it was decided to construct a new port five miles beyond Klang. The site chosen was a mangrove swamp partially submerged by every high tide; and for the reclamation of this, the making of railway embankments and the construction of approaches to the wharves and building sites, earth had to be brought from a distance. While the work was in progress, the coolie labourers employed suffered severely from malaria, which increased in severity as the work approached completion. The health of the town was also affected, and when at last the new port and railway were opened on September 15, 1901, and a large number of Government servants and others connected with the shipping were transferred from Klang to quarters at Port Swettenham, many of them contracted malaria.

This appears to have seriously alarmed the authorities, who had remained unmoved at the fearful mortality that had taken place before the opening of the new port, and, as a result, steps were taken to draw up a scheme for the drainage of both stations. Meanwhile, quinine was freely administered as a prophylactic with such good effect that a marked improvement was recorded even before the drainage schemes could be executed. The epidemic which had raised the mortality to such a frightful figure in 1900 and 1901 declined very rapidly after the opening of the port and railway, and the total number of deaths during 1902 was less than one-quarter of those in 1901.

To those acquainted with conditions in the tropics, the occurrence of an epidemic of malaria during the construction of a new port and railway, and its disappearance after the completion of the work, occasions no surprise, for it is just what has happened hundreds of times in the past.

In the East, the coolie labourers employed on such works are drawn almost entirely from the poorest and most ignorant classes of the population; they are often brought long distances and set down in a country in which the climate, and even the food obtainable, differs greatly from that to which they are accustomed; if housed at all, they are generally crowded into temporary huts, but frequently they are left to find what shelter they can; they have often no one to look after them when sick, and no means of obtaining food if for any reason they are unable to work. Their work is arduous and their pay small, and owing to the fact that they generally try to save money for the support of dependants at home, it is no uncommon thing to find them attempting to exist upon a miserably insufficient diet. Camped as they usually are upon the site of the work, their surroundings are almost always highly insanitary, for there is rarely any pretence at a conservancy system, and sometimes no proper water supply; and if much earth-work is going on, the numerous pools of water formed during the rainy season speedily become the breeding places of countless swarms of mosquitoes. In these circumstances it is not surprising that coolie labourers on large public works should be decimated by outbreaks of epidemic disease. But whatever happens to the coolies, the work has still to go forward, so that as long as it is in progress there is continual immigration of new labourers to fill the gaps caused in the labour force by sickness, desertion, and death. This continual immigration is a further source of mischief, for the constant introduction of gangs of susceptible newcomers into camps which have already become hotbeds of disease increases the trouble, just as the addition of fuel to a glowing fire increases the blaze.

Those who, like the writer of this letter, have watched the course of epidemics of this kind among Indian coolies, cannot fail to trace in the history of events at Klang and Port Swettenham a similar occurrence. It may be remarked that the epidemic at these places began among the labourers on the work, and increased in severity as that work progressed; it occasioned a fearful mortality, such as is never seen except in conditions similar to those described above; and just as its origin can be traced to the construction of the new port and railway, so also can its decline be traced to the completion of this work. Once a big project of this kind is finished, the labour force rapidly disperses, immigration of the class of labour employed ceases, a settled population takes its place, and conditions as regards health rapidly approach the normal once more.

In these circumstances it becomes difficult, if not impossible, to estimate the value of the drainage projects that were carried out at Klang and Port Swettenham after the decline of the epidemic, for it is certain that in any case there was bound to be a great improvement after the completion of the work on the new port and railway, and the dispersal of the labour force engaged on their construction. The only legitimate method of testing the results of the drainage of the two stations is the comparison of the mortality rates of Klang prior to the commencement of construction work for the new port and railway with those recorded after the introduction of the drainage schemes, and until this has been done it is misleading to claim these places as demonstrating the value of drainage in combating malaria.

As for the statement that more than 400 lives per annum have been saved by the drainage of Klang and Swettenham, it is an absurd fiction based on the ridiculous assumption that but for the drainage schemes the enormous rate of mortality recorded in 1901 would have continued unchecked. CHAS. A. BENTLEY.

Bombay, October 22, 1910.

I HAVE read with interest Dr. Bentley's interpretation of the figures relating to the anti-malarial works at Klang and Port Swettenham published by Dr. Travers and myself, and I am familiar with Dr. Bentley's valuable paper on the human factor in malaria. This factor is undoubtedly of profound importance in certain outbreaks; but at Klang and Port Swettenham the sequence of events was not to be explained by Dr. Bentley's theory, and he has, I think, overlooked two points in my published reports which at once set aside his explanation of the figures.

The suggestion is that the origin and sustaining factor in the outbreak was a camp at Port Swettenham densely populated with coolie labour, the coolies of which not only suffered severely from malaria, but also infected the other inhabitants of the port and of Klang, and that when the construction works at the port were finished at the end of 1901, the camp was broken up, the coolies dispersed, and the epidemic subsided.

If this theory be correct, we should naturally expect to find evidence of a severe outbreak among construction coolies at Port Swettenham during 1901.

As a matter of fact, not only was there no outbreak of malaria among coolie labourers at Port Swettenham, but there were actually no camp coolies. There is abundant evidence that this was the state of affairs, although, as the port was not opened until the end of the year, it was not unnatural for Dr. Bentley to suppose such a camp existed.

To explain, I may say that most of the earth construction was finished at the end of 1895 (Dr. Travers gives the date of beginning work as 1897, but this is a slip; see *Journal of Tropical Medicine*, 1903). The wharves were finished at the end of 1900. The only works then remaining to complete the port were three iron goods-sheds and the passenger railway station, for which skilled labour was required. This was not forthcoming, and these few buildings were not finished until September 15, 1901, when the port was opened. Through trains were, however, running from June, 1901.

On my first visit to Port Swettenham, in January, 1901, instead of finding a great camp of coolies as suggested by Dr. Bentley, I found only some twenty or thirty persons. Instead of poorly paid coolie labourers, there were high-paid Chinese artisans. Except for these few Chinese, occupying one of a series of empty huts formerly occupied by the construction coolies, the place was deserted. The Government quarters on each side of the main road also stood empty, and were not to be occupied until some nine months after my first visit to the port.

This is a condition of affairs so completely different from what is demanded by Dr. Bentley's theory, that the theory obviously becomes untenable; and to show that such was actually the state of affairs in 1901, I now give some extracts from published reports.

A.—*Evidence of date of beginning works.*

"The extension of the Klang line from Klang to Kuala Klang (former name of the Port) was commenced on January 1, 1895, and at the date of writing (April, 1896) is nearing completion. It is not proposed to open this line until the wharves at the mouth of the river have been completed" (see Report of Resident Engineer, Railways for 1895, Selangor Government Gazette, 1896).

B.—*Evidence that wharves were completed at the end of* 1900, *and that remaining work was for artisans, who were difficult to obtain and keep at Port Swettenham.*

"The works remaining on hand are the passenger station and goods-sheds, which are being pushed on as fast as the difficulty of keeping skilled labour at this unpopular spot will permit. It is anticipated that the whole of the work will be completed by the middle of the present year" (see Administration Report for 1900, Selangor Government Gazette, 1901).

C.—*Evidence that artisans were not poorly paid and were scarce.*

Under the head of "Labour" in the Administration Report on Selangor for the year 1900 (Selangor Government Gazette, 1901), the British Resident wrote:—"More serious difficulty is caused by the great dearth of artisan and mechanical labour, the available number of skilled workmen having declined rather than advanced. . . . The mechanic of this country is almost always a Chinaman. He can do good work, and is well paid for it, and is consequently very independent. Latterly employers even of their own nationality have been quite unable to get the men they require, and works of all description have suffered in consequence."

D.—*Evidence that there was no great coolie population at Port Swettenham in* 1901.

"I was not surprised that the construction coolies had suffered severely from malaria, and although at the beginning of 1901 few coolies were actually living in Port Swettenham, I formed a very unfavourable opinion of it owing to the short residence necessary to infect new-comers." This is an extract from my Official Report to Government on the anti-malarial works at Klang and Port Swettenham. It was published in the Selangor Government Gazette, April, 1905, and reprinted in the Indian Medical Gazette of September, 1905. Dr. Bentley has, I think, overlooked this report.

E.—*Evidence that there was no great epidemic of malaria at Port Swettenham in the early half of* 1901.

If there was a great epidemic of malaria at Port Swettenham among coolies in 1901, one would expect to find evidence of it in the admissions to hospital. Now, in my articles in the *Journal of Tropical Medicine* in November and December, 1903, and April, 1905, a table appears giving the monthly admissions to Klang Hospital of cases of malaria from the port. The numbers were as follows:—January, 3; February, 0; March, 3; April, 4; May, 0; June, 5. During the six months, consequently, which Dr. Bentley supposes to have seen the culmination of a malaria outburst which almost annihilated Klang and Port Swettenham, the cases of malaria admitted to the hospital from the vortex of the storm numbered no more than 15. Dr. Bentley can scarcely have noticed these figures.

So far from Dr. Bentley's suggestion being correct, the figures show that the construction coolies had left Port Swettenham long before the great outbreak arose among the Government population, and those connected with the shipping who went to reside there after the port was opened. The epidemic at Port Swettenham, which arose in 1901 after the port was opened, was not among, or in any way connected with, poorly paid construction coolies, but among Government servants and their families, the shopkeepers, and the well-paid loading labourers (180 in number) who were transferred from Klang. Beyond these there was no population at the port. The outbreak began nine months at least, to my personal knowledge, after the construction coolies had left, and consequently it is impossible to attribute the subsidence of the outbreak to their departure.

With regard to Klang town, I cannot quite gather whether Dr. Bentley means it to be understood (*a*) that construction works and a coolie camp existed at Klang as well as at Port Swettenham, or (*b*) that the coolies from Port Swettenham infected Klang town.

If the former, I may say at once that no such works or such a coolie camp existed at Klang.

If the latter, why should Klang, formerly so susceptible of infection, be now immune? In the nine years which have passed since the outbreak in Klang, there has been an enormous increase in the populations of the towns and of the district. Places with much larger populations than that of Port Swettenham in 1901, or even to-day, and places much nearer than Port Swettenham to Klang, have been devastated with epidemics of malaria. Malarial subjects from these surrounding neighbourhoods now enter Klang in far greater numbers than nine years ago from Port Swettenham. Yet the residents of Klang do not suffer from malaria, and statistics show that malaria is hardly ever contracted in Klang to-day. Of 455 Klang children examined in 1909, thirteen only showed evidence of malaria. Of these thirteen, not one but had recently come to Klang, and had a history of malaria before arrival in the town.

I may remark, in passing, that when he asserts that the authorities remained inactive until "alarmed by the fearful mortality" at the new port, Dr. Bentley does less than justice to Dr. Travers and the administration, since the Klang scheme, which was to prove so dramatic a success, had already been approved before the occurrence of the Port Swettenham outbreak.

Dr. Bentley also suggests that the credit for the improvement of the health of Klang may be ascribed to quinine. Quinine, as a matter of fact, was freely administered at Port Swettenham in 1901, but no public distribution of quinine as a prophylactic was undertaken at Klang.

Not a single grain during all these years was given except to my hospital and private patients actually suffering from malaria.

Dr. Bentley thinks it a "ridiculous assumption" that the high death rates of 1900 and 1901 would have remained unchecked but for the measures recorded. If he will do me the honour of reading my forthcoming book, he will find it proved (as I venture to claim beyond dispute) that in many places in the neighbourhood of Klang, so long as the Anopheline factor remains undisturbed, these high death-rates do, alas! without any "absurd fiction," continue. In view of the large increase of population, we are justified in assuming that, but for the measures taken, malaria would have claimed many more than four hundred victims during each of the years under review; and Dr. Bentley will find that the two cases of anti-malarial works, the reports of which have incurred his criticism, have been paralleled strikingly in numerous instances.

It is commonly assumed that to rid even small tropical towns of malaria by anti-malarial operations presents insurmountable difficulties, and that to attempt such a campaign over extensive rural areas would be tilting at windmills. It can be shown, however, that in the Federated Malay States planters have, quite unconsciously, been carrying out great anti-malarial works over far greater areas than Klang and Port Swettenham, for the benefit of much larger populations, at only a small fraction of the expense, and with complete success. Indeed, one cannot help suspecting that great anti-malarial works are constantly being carried to a successful conclusion by those whose last idea would be that they were carrying out great sanitary works; and in view of what has already been achieved it would be rash to deny that the future may have in store for us the final expulsion of malaria from the whole of what is, even to-day, one of the most malarious portions of the tropics.

MALCOLM WATSON.

Klang, Federated Malay States, December 27, 1910.

Studies of Magnetic Disturbances.

IN the number of NATURE for August 11, 1910, Dr. L. A. Bauer published some results of investigations of magnetic disturbances, forming an abstract of papers published in *Terrestrial Magnetism*, xv., Nos. 1 and 2.

In these papers Dr. Bauer treats a number of problems of the greatest interest for the study of terrestrial magnetism; but in my opinion there are certain points of a fundamental nature which it would be well to take up for discussion, partly because certain of his results differ considerably from those of other investigators in this field.

Dr. Bauer treats the "positive equatorial storms" of May 8, 1902, and January 26, 1903, and concludes from a mathematical analysis "that for both disturbances the systems of disturbance forces which it would be necessary to superpose upon the earth's own magnetic field were precisely of the same character as the earth's. In other words, were we to assume electric currents as constituting the disturbance systems, then, as in the case of the earth's field, the currents would have to circulate from east to west if they are positive ones and in the contrary direction—from west to east—if they are negative or such as would be produced by moving negative charges. Furthermore, for both disturbances the electric currents would have to circulate chiefly in the regions above the earth" (NATURE, *loc. cit.*, p. 192).

I am not quite sure of what is meant by the expression, that the disturbance field is precisely of the same *character* as the earth's own field. For a real similarity, which would allow conclusions to a similarity in origin of the two phenomena, we should have provided the following relation

$$\mathbf{P}=k\mathbf{T} \quad . \; . \; . \; . \; . \; . \; . \quad (1)$$

where $\mathbf{P}$ is a vector representing the perturbing force at a certain place, $\mathbf{T}$ is the total force of the earth's permanent magnetic field at the same place, and k is a constant. The relation (1), however, is not even approximately fulfilled for any of the principal groups of disturbances treated by Birkeland in his work, "The Norwegian Aurora Polaris Expedition, 1902–1903," not even for the equatorial storms considered in Dr. Bauer's paper, so that the similarity assumed by Dr. Bauer cannot mean that relation (1) is fulfilled.

Then when Dr. Bauer, from the similarity between the earth's own field and the field of the positive polar storms, concludes that the disturbance current systems must circulate round the earth in the same direction as that which may be supposed to produce the permanent field of the earth, this conclusion is not justified without further proof, for the two similar fields show great differences. While the forces $\mathbf{P}$ and $\mathbf{T}$ have the same direction near the equator, this is no longer the case nearer the poles, where their vertical components have opposite directions.

It seems also difficult to understand how the direction found by Dr. Bauer can be brought into harmony with his statement that the greatest part of the current is to be found above the surface of the earth. For suppose that an electric current—circulating around the earth and *above its surface*—shall produce the observed perturbing force directed towards the north, it follows from Ampere's rule that the current must pass from *west* to *east*, or in the direction opposite to that found by Dr. Bauer. So long as we regard external systems as forming the primary cause of disturbances, this result must hold even if we take into account the magnetic permeability of the earth and the effect of induced currents.

Dr. Bauer states that the disturbance systems probably always can be supposed to originate from one internal and one external current system, and for the equatorial storms he has given the mathematical method for separating the two systems.

The separation was carried out for the perturbation of May 8, 1902, and "the surprising result revealed itself that the internal currents went in the same direction as the external ones, the latter being about three times the strength of the former."

Now two systems of this kind, keeping on simultaneously, must in some way be physically connected; but it seems rather difficult to see how this connection could be produced, for, as Dr. Bauer himself rightly remarks, the internal currents cannot be induced currents. Thus it seems that merely simple physical considerations will make the result very improbable, and besides, I think that objections can be made to the way in which it is deduced.

I am not going into details regarding the validity of the mathematical method, but I am merely going to show by an example that the method used by Dr. Bauer cannot be trustworthy.

The equations for the internal systems (see *Terr. Mag.*, xv., No. 1, p. 26) are simply expressing the forces on the surface of a sphere uniformly magnetised along a certain diameter. Suppose, now, that in a plane, perpendicular to this diameter and passing through the centre of the sphere, there was a circular current concentric with the sphere and with a radius large compared with that of the sphere. The magnetic field of the current in the space occupied by the sphere would be nearly uniform, and if the sphere was made of a homogeneous material it would be *uniformly magnetised*. In other words, the equations which should express a field due to an *internal* system will in this case express a field actually caused by an *external* one. The system of equations, however, which according to Dr. Bauer should express the field due to an external system, will not even approximately be able to express an external system of the kind here supposed. I think this will be sufficient to show that the result of the separation will require further proof.

I should also like to say a few words regarding the direction of the circular current of negative corpuscles which may be formed round the earth in the plane of the magnetic equator. These currents play an important part in the kathode ray theory of Prof. Birkeland, and it is of importance that no misunderstanding should exist on this point.

Dr. Bauer arrives at the conclusion that the direction of motion of the negative corpuscles of such a current is from *east* to *west*. I have not from his short note been able to follow his argument, but a simple consideration will show that the corpuscles will encircle the earth in a direction opposite to that found by Dr. Bauer.

Suppose a negative corpuscle is moving in the plane of